

LETTERS TO THE EDITOR.

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The Origin of Radium.

IN replying to Mr. Soddy's communication published in NATURE of June 13 (p. 150) I cannot refrain from expressing my regret on learning that he has apparently taken my paper in the *American Journal of Science* for December, 1906, as such a serious "criticism" and "imputation" on my part in dealing with his paper on "The Production of Radium from Uranium." It was my intention merely to point out certain conditions of experiment which appeared to me to be quite essential to the solving of the problem of the growth of radium in uranium compounds, conditions which had apparently been neglected in his own experiments, and to show that where these conditions had been fulfilled the results were not in agreement with those which had been obtained by him. I was then, and am still, of the opinion that the experimental procedure which Mr. Soddy adopted was not suited to give conclusive results of either a positive or negative character, and this opinion is certainly strengthened by the fact that, under very similar conditions of experiment, Prof. Rutherford was unable (Bakerian Lecture, Phil. Trans., 204, 218) to observe the growth of radium in a solution of Giesel's "emanating substance," although he has since concluded that radium was actually being produced in quite notable quantities.

My suggestion of the "accidental and unconscious introduction" of radium salts during Mr. Soddy's tests was only one of a number of possible sources of error to which I have already directed attention. That the other suggestions have not met with his disapproval, and that he is at least convinced of the necessity of starting with purified uranium salts, would seem probable from his statement that he is now continuing the investigation with purified uranium compounds. I am very glad to learn that the results which he has now obtained entirely confirm and extend the results which I have already published. It may be of interest to add that my original solution of one hundred grams of purified uranium nitrate has recently been tested and found, after a period of more than two and one-half years, to contain less than 10^{-11} gram of radium.

"The experiments described in this paper are considered to indicate that the results obtained by Mr. Soddy are without significance and that one or more products of a slow rate of change intervene between uranium and radium." This is the particular paragraph to which Mr. Soddy now raises an objection. I fully realise that this statement is open to criticism; it was an unsuccessful effort at brevity. A longer but more satisfactory summary would perhaps have been:—The results of the experiments described in this paper are in support of the hypothesis that one or more products having a slow rate of change intervene between uranium and radium, and indicate that the results obtained by Mr. Soddy are without quantitative significance in so far as they relate to the production of radium by uranium.

"Commercial salts" of uranium may contain, and usually do contain, quite appreciable amounts of every constituent of the minerals from which they have been prepared. The presence in such salts of a small proportion of the immediate parent substance from which radium is derived is therefore in itself no indication of any genetic connection whatever between uranium and radium. My observation of the growth of radium in actinium preparations, even if it has served no other useful purpose, has certainly indicated where the immediate parent of radium is to be sought. To judge from the results which I have obtained in recent experiments along the same lines it would appear that, unless the rate of disintegration of radium now assumed is greatly in error, the chemical process outlined in my "Note on the Production of Radium from Actinium" is capable of effecting the essen-

tially quantitative separation of the radium parent from most of the other substances present in a uranium mineral.

In conclusion, it may be desirable to direct attention to the fact that the only evidence we now have that radium is a disintegration product of uranium is the constancy of the ratio between the quantities of these two elements in the natural minerals, a relation which was first pointed out in these columns by the writer. BERTRAM B. BOLTWOOD.

Yale University, New Haven, Conn., June 29.

The "Double Drift" Theory of Star Motions.

I HAVE been greatly interested in Mr. Eddington's account in NATURE of July 11 (p. 248) of Prof. J. C. Kapteyn's investigations of this subject. Although I do not quite follow his argument for the existence of two overlapping systems of stars (more dramatically termed "two Universes" by Prof. Turner), I yet venture to suggest an explanation of the apparently (perhaps really) opposite "drifts," which seems to me to agree sufficiently with the observed facts.

If we adopt Lord Kelvin's postulate of a single vast stellar universe very slowly condensing towards its common centre of gravity, we might expect that the component stars would move for the most part in ellipses or spirals of very varying degrees of eccentricity and of inclination to the mean orbit—perhaps indicated by the Milky Way. If we further postulate (what is very generally admitted) that our sun is situated towards the central rather than towards the outer portion of the whole system, then, just as the planets, through differential angular motions as regards the earth, appear sometimes to move in a retrograde direction or to be quite stationary, so a certain proportion of the stars might be expected, at any given period, to exhibit the same phenomena.

But further, considering the enormous distances that are known to separate the stars and star-groups from each other and the extreme slowness of their angular motions, there seems no reason why their respective orbits should not be almost as frequently in a right-hand as in a left-hand direction in regard to the central plane of general motion.

Our knowledge of the actual motions of the stars may not inaptly be compared to what astronomers would possess of the solar system supposing the whole of their observations had been limited to a period of about twenty-four hours, and that the sun was invisible. The motions of the planets and their satellites thus determined would seem as strange and incomprehensible as do those of the stars at the present time, our accurate observations of which have been limited to a few centuries.

It will probably be of interest to many of your readers (as it certainly will be to myself) if some of your mathematical correspondents will explain why, and in what way, some such system as is here suggested is incompatible with the facts set forth by Prof. Kapteyn and others.

ALFRED R. WALLACE.

IN the article to which Dr. A. R. Wallace refers, and elsewhere, I have confined myself to attempting to establish the result that the stars distribute themselves into two systems according to their motions, abstaining as far as possible from defining what physical connection is implied by the rather vague word "system." Whether the two systems are comparatively permanent and have come together from different parts of space, or whether they may have been evolved from a single system, is, in the present state of our knowledge, a somewhat speculative question, and it is with some reluctance that I enter upon it. Still, without asserting that the hypothesis of two permanent systems is the only possible one, I know at present of no other satisfactory explanation. In the system suggested by Dr. Wallace (in which the stars move about the centre of the universe in ellipses, some forward and some retrograde, with all sorts of eccentricities) the motions would be for our purposes haphazard. Thus the system would form a single and not a double drift; the extremely eccentric orbits form a perfect transition between the direct and retrograde orbits. To account for two drifts, it is not sufficient to show that some stars move forward and some backward; it must be shown that there is a concentration of the motions about two definite veloci-

ties (definite in magnitude and direction), and it does not appear to me that the suggested system provides for this. In fact, it is difficult to see how gravitation towards the centre of the universe could separate the motions of the stars into two systems, if they originally formed one system.

A. S. EDDINGTON.

Royal Observatory, Greenwich, July 18.

The Dental Formula of *Orycteropus*.

NORMALLY the adult *Orycteropus* has in each jaw but five teeth, though frequently, especially in young animals, a number of smaller teeth are found further forward. In 1890, Mr. Thomas discovered in both the upper and lower jaws of fairly large foetal specimens a number of milk-teeth, seven in the upper and four in the lower jaw. So far as I am aware, nothing further has been discovered regarding the dental succession.

In the skull of a newly-born specimen which I have been enabled to study through the kindness of Dr. Perinquey, of the S. African Museum, I have been fortunate in finding a full set of milk-teeth in both upper and lower jaws. In the upper are three minute but calcified incisors, one canine and six premolars. Of these only the last five premolars probably cut the gum, and only the fourth and sixth are large enough to be functional to a slight extent. Succeeding teeth are found under the third, fourth, fifth and sixth premolars, and possibly under the second. Beyond the sixth premolar there is evidence of at least four true molars. In the lower jaw there are also three minute calcified incisors, one minute canine, and six milk-premolars. Of these the second, third, fourth, fifth, and sixth premolars probably cut the gum, and are slightly functional. The germs of replacing teeth are found in connection with all the premolars except the first. Behind the last premolar are evidences of five true molars. The dental formula of *Orycteropus* may thus be taken to be:—

Incisors	Canine	Premolars	Molars
$\frac{1}{1} \frac{2}{2} \frac{3}{3}$	$\frac{1}{1}$	$\frac{2}{2} \frac{3}{3} \frac{4}{4} \frac{5}{5} \frac{6}{6}$	$\frac{1}{1} \frac{2}{2} \frac{3}{3} \frac{4}{4} \frac{5}{5}$
$\frac{1}{1} \frac{2}{2} \frac{3}{3}$	$\frac{1}{1}$	$\frac{1}{1} \frac{2}{2} \frac{3}{3} \frac{4}{4} \frac{5}{5} \frac{6}{6}$	$\frac{1}{1} \frac{2}{2} \frac{3}{3} \frac{4}{4} \frac{5}{5}$

This dental formula is quite unlike that in any living mammal, but if we assume that the ancestor of *Orycteropus* had functional succeeding incisors, and canines, it would have had a formula not at all unlike that found in many of the Mesozoic mammals. Elliot Smith suggests that it may have branched off very early from the subungulate stem. Kitchen Parker was more impressed with the resemblances of the skull to that of the marsupials and lower insectivores.

Some further light may be obtained by a careful microscopic examination of the developing teeth, which I hope to undertake immediately.

R. BROOM.

Victoria College, Stellenbosch, June 25.

THE RADIO-TELEGRAPHIC CONVENTION.

THE report of the select committee appointed to consider the radio-telegraphic convention drawn up by the Powers in November last has just been published as a parliamentary paper. The committee recommends, by a majority of five to four, the ratification of the convention, a result which will hardly surprise those who have followed the evidence given before the committee, though the narrowness of the majority may be difficult to understand.

The provisions of the convention have already been summarised in NATURE (vol. lxxv., p. 59, November 15, 1906), so that it will not be necessary to repeat them here. It will be recollected that it was then pointed out that the provision of prime importance, and the only one likely to lead to opposition to the ratification of the convention, was the one requiring that 'coast stations and ship stations are bound to exchange radio-telegrams reciprocally without regard to the particular system of radio-telegraphy adopted

by these stations." The necessity for this provision and the highly beneficial results likely to accrue from its enforcement to civilisation and maritime interests were described, and the hope was expressed that the private interests of the Marconi Company would not stand in the way of its adoption.

A study of the evidence presented to the committee and clearly summarised in its report shows that the only opposition to ratification came from those representing the interests of the Marconi Company. They, having already secured what amounts to a practical monopoly so far as Great Britain, Italy, and Canada are concerned, are not unnaturally desirous of maintaining and increasing that monopoly. Whether the policy of not ratifying the convention which they support is likely to lead to such a result seems more than doubtful. The evidence shows that, so far as the world as a whole is concerned, the Marconi Company do not possess even a majority of existing stations, but only about one-third of the total number.

The ratification of the convention by all the signatory Powers except Great Britain would inevitably lead to a growth of other systems at the expense of Marconi stations: existing Marconi stations under their control would necessarily be discontinued unless they consented to acquiesce in the provision for intercommunication. The numerous stations existing along the south coast of England, if they refused to intercommunicate, would be useless for the shipping of foreign nations using other systems, and the necessity for the erection of other stations in their place on the north coast of the Continent would arise. If these, as is probable, interfered with the working of the English stations, protest would be useless from a country outside the convention. From almost all points of view it seems, as a matter of fact, that the Marconi Company stands to gain rather than to lose by the adoption of the convention by Great Britain.

Of the technical objections raised by the Marconi Company little need be said. Since the representatives of all the other systems were agreed that there exist no real difficulties in intercommunication from the technical standpoint, one is compelled to the conclusion that these objections are biased by other considerations, unless, indeed, the Marconi system is so inferior to all others that it alone possesses this great disadvantage.

It will be recollected, probably, that great stress was laid by many writers in the daily Press at the time of the international Conference on the naval and military aspects of the convention, and Great Britain was represented by some as handing herself over bound to the Powers. That these contentions were entirely without foundation was pointed out in NATURE (*loc. cit.*), and would have been clear to anyone who took the pains to study the actual provisions of the convention. The section of the report of the select committee dealing with this aspect of the question should be sufficient to dispel any lingering doubts which may still remain.

Wireless telegraphy has been very much before the public for the past ten years. In sensational achievement much has been accomplished, and of recent years it has figured somewhat largely as an international bone of contention. But the practical commercial development has been disappointingly slow. It is to be hoped that with the ratification of the convention a period of peaceful progress may ensue, and that some of the well-deserved fruits of many years of patient experimenting may be gathered by the numerous inventors who have been working in this field.

MAURICE SOLOMON.